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REPLY

Dr. Veenhuyzen argues that inserting dual-chamber implantable cardioverter-defibrillators (ICDs) in the interest of cost-saving is unethical, because those patients who would not go on to require an upgrade are asked to "shoulder the burden of unnecessary medical procedures" by having the dual-chamber device implanted upfront. This argument rests on the assumption that the risks of atrial lead implantation outweigh the benefits of the dual-chamber device. However, the benefits are likely greater than outlined in Dr. Veenhuyzen's letter.

As described and referenced in our study (1), although not all studies show improved discrimination between ventricular and supraventricular arrhythmias with the dual-chamber device, most do. Further, even if there were no benefit, the issue of risk is more complicated than that described. There is an increased risk of atrial lead dislodgement with the dual-chamber device (other complications are not different). However, as referenced in our study, there is also an increased risk of infection, a more serious complication, with upgrade. Even if there were no benefit at all, patients might trade an upfront risk of a less-serious complication to avoid the possibility of a more serious risk later on. Thus, we believe that our data show not that a strategy of universal dual-chamber ICD placement would trade clinical good for cost-saving, but rather that the most beneficial approach for the patient is also the least expensive for the health care system.

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Doppler Echocardiographic Profile and Indexes

We read with interest the study published recently in the *Journal* by Tan et al. (1). We have several concerns.

First, the investigators point out in the Limitations section that the modified Bernoulli equation was not used in their study. However, Tan and colleagues do not give a justifiable reason for this in the cases studied. Did all these patients have a pre-coarctation velocity (i.e., V_1) that was <1 m/s? Our concern is that this approach may mislead clinicians. Giving only a brief passing mention for the necessity for the modified Bernoulli equation in the Limitations section is troubling and may lead to misconceptions in the way this lesion is assessed in the cardiology community.

Second, and more important, the researchers mention that the peak systolic pressure gradient (SPG) is dependent on aortic compliance, and they reference a study by Tacy et al. (2). However, there is no mention that the measurement they propose clinicians use (diastolic velocity [DV]) may also be related to compliance as well. Experiments performed by Tacy et al. (2) and in our laboratory (3) suggest that diastolic runoff, and thus DV, depend on vessel compliance. To understand this point one simply has to take the argument to the extremes. If the entire aorta is noncompliant then there can be no diastolic runoff, even considering the most severe coarctation lesion. Of course, such a condition for the entire aorta is unlikely to exist, but it is useful to consider to demonstrate the concept of the relationship between diastolic runoff and aortic compliance. Taking several steps back from this extreme, if one considers the complexities to flow that occur with a stiff stent in place after intervention, it is clear that significantly more investigations are needed before such parameters as DV are freely used in clinical practice.

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REPLY

The comment given in the Limitations section of our study (1) regarding the modified Bernoulli equation was aimed at highlighting the fact that the equation should not be used for low velocities owing to mathematical properties of the exponential curves.

If we make the simple assumption that the flow through the coarctation is dominated by resistance, then the flow is determined by that resistance and the pressure difference across it. Thus,